has done more to attract the favorable attention of the profession abroad. Most heartily do we congratulate Dr. Hammond on his well-earned success in the department of medicine he has wisely chosen to cultivate.

III.—THE WEST RIDING LUNATIC ASYLUM REPORTS.

THE WEST RIDING LUNATIC ASYLUM MEDICAL REPORTS. Edited by J. Crichton Browne, M. D., F. R. S. E. Vol. V. London, Smith, Elder & Co., 1875; 292 pages.

Dr. Crichton Browne and the able corps of medical men he gathered about him at the West Riding Asylum, offer a wholesome example to others in like circumstances for the observation of mental and nervous disorders. In this volume, the fifth of the series, we have a collection of papers, real contributions to human knowledge, on various subjects connected with mental and nervous pathology and physiology and the histology of the nervous system. The majority of these papers are by the officers of the West Riding Asylum, or those lately connected with that institution; a portion are by other neurologists, like Prof. Ferrier, Dr. Hughlings Jackson, and Dr. J. Milner Fothergill, who find this work a convenient and useful medium for the publication of some of the best results of their labors.

Of the fifteen articles or memoirs contained in this volume, we can give detailed attention to only a few in the present notice; others will be given in abstract more or less fully in the 'periscope of this and succeeding numbers of the JOURNAL. Nor shall we follow the order in which they appear in the volume, or confine ourselves solely to those which are perhaps the most important as regards subject or treatment, but will rather notice more some of those points which seem most to call for remark or criticism.

Dr. Hughlings Jackson's contribution to this volume is upon a subject important both to the medical man and the jurist, that of "the temporary mental disorders after epileptic paroxysms." We quote his title, which in a measure exhibits his peculiar views in regard to this form of attack, not, however, intending to appear to coincide with them altogether. We shall notice this point, however, a little further on.

Speaking of the frequent definition of epilepsy in which loss of consciousness is considered as an essential part of the paroxysm, Dr. Jackson, while accepting it for present purposes, gives his own, which, though it can be gathered from his writings elsewhere, may, perhaps, as well be stated here. He says: "The epilepsy of authorities is to me but *one* epilepsy; the paroxysms in it are, I consider, results of discharging lesions beginning in the *very highest* nervous centres of the cerebral hemispheres. Other epilepsies (such seizures, for example, as are usually called epileptiform,) differ, in that in them the discharging lesion is of nervous arrangements of subordinate centres in the cerebral hemisphere. In them, consciousness is lost late or not at all. But even empirically and for practical purposes (that is from the symptomatic or from the clinical point of view,) the distinction should not be into genuine epilepsy and epileptiform seizure; it should be into cases in which loss of consciousness is the first event, and cases in which it occurs early, cases in which it occurs late, or not at all." We have quoted this definition so fully, though it occupies only a foot-note in the book, because there are few investigators who have given more attention to the phenomena of this disease than Dr. Hughlings Jackson, not because we agree with it. The theory of "discharging lesions" originated and maintained by him, is far from being generally accepted as applying to all cases of epilepsy, and his definition does not readily account for the common forms of so-called reflex epilepsy arising from peripheral irritations. There is a question, also, whether he is correct in accepting as the usual definition one that excludes cases without loss or disorder of consciousness from the category of epilepsies,—such is not the view of some of the high authorities who have recently written upon the subject, to say nothing of the extension of the idea that has led some authors to speak of "spinal" epilepsies.

The peculiar idea presented in this article by Dr. Jackson, and which is intimated in the title he gives it, is that epileptic insanity, the raving paroxysms, etc., sometimes observed in epileptics, occurs only after an epileptic attack, *i. e.*, it does not absolutely replace the attack as is held by many writers. He thus rejects the hypothesis of larvated or masked epilepsy of Falret. The grounds on which he adopts this way of thinking are not fully given; he only says that "he does not now think it possible that a nervous discharge at all comparable in degree to that which causes convulsion, would cause even such caricatures of normal action as occur either in epileptic mania, or in slighter cases of mental automatism," and thus while he admits that no signs of the fit may be discoverable in some cases, he is still inclined to always infer its prior occurrence. He holds that there is during this paroxysm an internal discharge too slight to cause obvious external effects, but strong enough to put out of use the higher nervous centres. This inaction of these centres relieves the lower centres of their controlling inhibitory checks, and hence the mental automatism, and the grotesque and dangeractions of epileptics. This is Dr. Jackson's view. He says: "The condition after the paroxysm is duplex: (1) there is loss or defect of consciousness, and there is (2) mental automatism. In other words there is (1) loss of control *permitting* (2) increased automatic action." In another place he explains the term "mental automatism," as used by himself; he uses the word *mental* because the doings are probably external signs of crude mental states—external signs of epileptic dreams.

Although the term automatism may be accepted by most in the signification here given it, we question its propriety when applied to such complicated actions as are often seen in epileptic insanity. Several very instructive, or at least suggestive, instances are related by the author in this article—actions which were apparently quite out of the usual routine of the patient's life, and which certainly seem to indicate some degree of mental activity and intention. It is not, in our opinion, sufficient to call these actions suggested, for that implies the activity of the perceptive centres, which excludes automatism in the strict sense of the word, as it likewise does unconsciousness. But without putting too fine a point on mere words, there seems to us no good reason for considering the acts of an insane epileptic as automatic reflex acts in the true sense of the word, and the instances here given do not appear to us to in any way properly support this view. The patients perform very complicated acts, indicating certainly the exercise of some of their reasoning powers, though judgment is in abeyance, and they have no recollection of their actions on their recovery. An automatic act, as we understand it, is properly a reflex act, complicated it may be, like the oaths of aphasic patients, but not one indicating complex mental processes, and if the term, "mental automatism," is intended to cover such cases when accompanied with temporary amnesia, we object to such unwarrantable extension of its use. There seems to us to be no more propriety in calling the ravings of epileptics automatic than in applying the same adjective to those of other insane persons, who likewise have frequently no recollection of their insane acts, after their recovery. The epileptic, after a paroxysm, is always or nearly always, in a dazed, confused condition, in which the higher faculties are more or less suppressed, and he is liable often to do the most insane acts, and have no recollection of them afterwards. Still, his condition at the time is not that of unconsciousness, though he subsequently has no recollection of his consciousness at the time.

A short paper on a rather interesting subject, that of the morbid histology of the brain in the lower animals, is contributed by Dr. Herbert C. Major, deputy Medical Director of the West Riding Asylum. He takes up the question, which, as he says, has never hitherto been answered, whether the same changes take place under morbid conditions in the nerve elements of the brain in lower animals, as in the human species. To answer this question he sought to discover whether the laws of development and decline that apply in man, are valid likewise in dogs and other animals. We can observe mental and moral changes in these animals occurring with advancing age, which are not altogether unlike the symptoms of senile decay in our own species. Following out this idea, he made very careful microscopic examinations of the brains of a dog, a horse, and a cat that had died or been killed at a very (for them) advanced age, and found changes in the nerve elements that bore a very decided resemblance to those found under similar conditions in the human subject. The inquiry is a very interesting and suggestive one in many regards, and we hope that it may be still farther taken up.

Dr. J. Crichton Browne gives an interesting paper on a physiological subject, the functions of the optic thalami. He begins by stating that the opinion long held on apparently somewhat insufficient grounds, that the optic thalami are the great centres or foci for the sensory nerves, has recently received important corroboration in the experiments conducted by Dr. Ferrier. The anatomical researches of Broadbent are also quoted as pointing this way, though the observations of Luys and others, and some still more recent ones, are not mentioned. The circulation of the brain, in its arrangement, is also referred to in support of this view, but the most of the proofs quoted by Dr. Crichton Browne are physiological and clinical rather than anatomical. He gives the history of a case in which the whole bulk of the right optic thalamus was involved in a destructive lesion, the adjoining parts being nearly intact. Still there were some lesions in the left thalamus which are not clinically accounted for in the paper. Other cases are reported, for comparison, in which other parts were implicated, either with the thalamus or separately. The criticisms we would make on these cases, as reported, is that the autopsies were not made minute enough to correspond with the advances in cerebral anatomy and physiology that have been made in late years. Thus it is hardly exact enough to describe "a great portion of the corpus striatum" as broken down, without saying which part is involved. The topography of the brain is now-a-days sufficiently well marked out, and so many physiological facts depend upon a recognition of the finer divisions, that a post-mortem in which they are ignored can hardly be said to be up to the requirements of modern science. Dr. Crichton Browne's autopsies, however, are perhaps not defective enough to deserve this criticism; he generally states what portion of the corpus striatum or thalamus is involved, but they are not as exactly stated as will certainly be demanded in the future with our increasing knowledge of the cerebral functions. The cases in all other respects appear to be well reported, and are interesting in connection with their discussion in this paper.

The greater part of this discussion is given up to the question of the affection of reflex excitability from lesions of the thalami. The remarks on this point are notable for the omission of all reference to the well known works of Meynert, who has considered these ganglia with the corpora quadrigemina as the chief centres for reflex action. Completely ignoring the systems of these eminent neurologists, Dr. Browne speculates on the fact of the abolition of the reflex excitability of the cord in these cases, and offers three theories, none of which, however, seem to him satisfactory, and he leaves the question still undecided. First, he suggests the possibility of the optic thalami being an inhibitory as well as a sensory centre, and gives more space to this hypothesis than it really deserves, considering the evident fact, as he himself observes, that an inhibitory centre can hardly be supposed to be set in action by being destroyed. Secondly, he supposes a subordinate inhibitory centre between the thalamus and the cord, a more reasonable hypothesis, but one that he rejects on the ground of the probable speedy exhaustion of such a centre. It seems to us more probable that other portions of the brain act as inhibitory centres, or at least there is no necessity for supposing such centre below the thalamus. A simple experiment would determine whether this loss of reflex excitability is due to inhibition,—severing the cord and watching for its re-establishment below the point of section—and this we thinkhas been done in effect, though we do not remember any direct experiments for this particular purpose. The only trouble would be, in case the reflex movements did not appear, to make certain whether or not the shock from the injury to the thalami had not affected them. And this brings us indirectly to the third hypothesis of our author. It is that the impressions that have reached the cord do not habitually react at once on the motor machinery, but are first transmitted to some higher centre, like the thalamus for example, to be re-transmitted downwards again. Thus, allowing the cord a certain degree of independent reflex activity, we must, according to this theory, consider that it mainly acts reflexly through a superior centre. Then, of course, if this centre be broken up, the cord being intact, the centripetal impulses are dissipated at the point whence they should be returned over motor routes.

In this theory Dr. Crichton Browne is, in our opinion, more nearly correct than in either of the others. In fact, it is about what would be suggested by an acquaintance with Meynert's theory of the function of the thalami, which, however, the author seems to lack, and it therefore may be considered original with him. There is a question, however, whether, as we have already intimated, the failure of reflex activity after destruction of the thalamus be not due to inhibition from other parts of the brain, rather than to what the author calls a dissipation of nervous energy. His statement that if the cord is cut across, the diffusion of energy is prevented and hence the reflex actions below the section are more perfect, appears hardly tenable, since diffusion of energy, it seems to us, might as well take place at such an interruption of continuity of the cord as by one at a higher centre. It is true, perhaps, that, as he suggests, if the cord be divided and no passage upwards is allowed, all the nerve force generated in the cord may be expended in reflex actions, which are thus intensified. But his idea of diffusion of nervous energy is not confined to this. He expressly says "if the cord remained intact and an interruption in continuity existed in the encephalon, then the centripetal fibres of the cord would convey upwards as usual, impressions derived from the periphery, in solicitation of aid or guidance from the encephalic centres, and that energy would be dissipated and wasted at the very point where it ought to have been reflected or turned backward." This passage, we think, gives evidence that the diffusion the author speaks of is not that over extensive nerve tracts. Moreover, in another place he says that the axis cylinder of a nerve has the power of giving out nerve force, and the greater the length of nerve traversed the greater the result obtained.

We object, therefore, to that portion of Dr. Browne's third hypothesis which attributes the loss of reflex excitability after lesion of the optic thalamus to dissipation of nerve energy on account of the intra-cerebral solution of continuity, while a similar spinal lesion produces no such effect. In our opinion, this lack of reflex action after lesion of the great reflex centres, is more probably due for the most part to cerebral inhibitory action over the spinal reflex centres, though other agencies may have some share in producing this effect.

Dr. Crichton Browne's paper is in many respects able, and exhibits much ingenious thought, and he comes very near to what we consider the most practicable solution of the principal question he takes up. But he seems to be much behind the times as regards knowledge of what has been done in relation to his subject. If he had only been familiar with the writings of Luys, for example, he would have probably considerably modified his article. It is, besides, tinged a little with what we may perhaps call the Spencerian ideas of nerve physiology, which seem to us to be altogether too popular with many authors, and this likewise appears to us a blemish.

Among other articles of value in the volume, which we can only mention here, is one by Dr. Ferrier, of London, on Menieres disease, or labyrinthian vertigo; in which he discusses very thoroughly the pathology of its symptoms; quite a lengthy one by Dr. Robert Lawson on the physiological action of hyoscyamine, and several others equally worthy of mention, but which must be passed over here. As we have already stated, abstracts of some of these will appear in the Periscope of this or future numbers of the Journal, and to that department we must refer our readers for a knowledge of their contents.

We will repeat here what we have already said in the commencement of this notice, that the example set by the chief and staff of the West Riding Asylum is one worthy to be followed by the officers of all similar institutions. There are probably few establishments in this country that afford such opportunities as the one which gives its name to the volume before us, but there are still fewer, if indeed there are any, that cannot contribute something new and of scientific and practical value to neurological medicine. One institution in this country, the Utica Asylum, furnishes a quota in the *American Journal of Insanity*, published and edited by its accomplished superintendent, Dr. Gray, and his able corps of assistants. In other places, also, with less means, careful scientific investigations have been taken up and successfully prosecuted, as, for example, by Dr. Kempster and his assistants at the Wisconsin Northern Asylum at Oshkosh. But as a rule we think American and foreign alienists have confined themselves largely to the practical duties of their positions, and their scientific contributions have been few and far between We do not say this in reproach, for we know that the burden of executive responsibilities placed upon asylum superintendents and physicians often precludes any attention to the less directly practical pursuits. We only hope that in the future it may be that with wider reading and knowledge on the part of the physicians, and more generous support on the part of the public, the magnificent opportunities afforded for the study of the pathology of the nervous system by these establishments may be further utilized for the benefit of mankind.

IV.—LECTURES ON DISEASES OF THE NERVOUS SYSTEM. BY JEROME K. BAUDUY, M. D., &c., &c. Reported by V. Biart, M. D. Revised and Edited by the Author. Philadelphia. J. B. Lippincott & Co. 1876. Pages 484.

Useful as this modest work is likely to prove to the general practitioner, we must express regret that it should have been published, until by wider reading and maturer reflection, it could have been brought nearer the present standard—none too high—of scientific and literary excellence. The feeling that the author is capable of better things, and will live to justify this feeling, induces us frankly to record this judgment.

The plan,—general make-up, of the book,—and we will add its spirit, are good, but it bears the marks on almost every page of haste, of a want of fine discrimination, of loose analysis, and of a lack of acquaintance with the valuable results of recent